

# **RESPONSE TO PUBLIC AND CASAC REVIEW COMMENTS**

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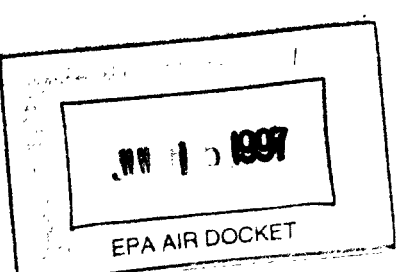
**Particulate Matter**

**Air Quality Criteria Document (AQCD)**

**CASAC Review Draft**

(November 1995)

U.S. Environmental Protection Agency  
National Center for Environmental Assessment  
Research Triangle Park, NC 27711



# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC 048	Festa/ America Forest and Paper Association	General	048-1	The database cited in the CD does not show consistent relative risks indicative of an association between PM <sub>2.5</sub> and adverse health effects.	The document discusses a large number of PM <sub>10</sub> studies which as a group make a strong statement. The document discusses the PM <sub>2.5</sub> data to include more recent publications, and conclusions related to the smaller FP data set are consistent with PM <sub>10</sub> studies. Appropriate conclusions are made concerning PM <sub>2.5</sub> health effects.	Kotchmar
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		General	048-2	The Agency has failed to critically evaluated the uncertainties and confounding factors.	The discussion in the revised document on uncertainties and confounding factors is a state of the art review of these factors. Extensive discussion and critical evaluation is provided and appropriate conclusions drawn with stated cautions. New publications and analysis are provided. CASAC reviewed this and considered it appropriate.	Kotchmar
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		General	048-3	No relationship has been established between the outdoor PM measurements and the personal exposure of the study subjects. The data show no effect for PM <sub>2.5</sub> .	The relationships that have been established are between ambient PM monitoring data and health effects. See response to comment 048-1 for PM <sub>2.5</sub> .	Kotchmar
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		General	048-4	The agency has failed to offer a biologically plausible mechanism for effects attributed to PM <sub>2.5</sub> .	The discussion in the document is well balanced and appropriate as indicated by CASAC.	Kotchmar
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		General	048-5	The agency should focus on research needs.	This subject is a separate effort. EPA presented research needs to CASAC for PM in a draft report in November 1996.	Kotchmar
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IIAEC-051	Letvin/ Kennecott		051-1-1	The conclusion section 1.8 does not reflect the content of the Executive Summary and should be converted to do so.	The conclusions in the Executive Summary were revised to reflect the final content of the chapters and for content of Chapter 1.	Kotchmar
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IIAEC-056	Leonard/GM	Chapter 5	056-5	Comments on sources.	Revised as appropriate.	Pinto

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IIAEC-047	Holmes/ CARB	Chapter 6	047-6-1	Since California has four out of the five Federal PM <sub>10</sub> non-attainment areas under the current standard, it is essential that air quality data from California and appropriate representation and analysis of those data be included in the Criteria Document. Based upon the material in the current version of the Criteria Document, it does not appear that the 7 years of dichotomous sampler data collected from up to 27 stations in California since 1989 was utilized. To the extent these data are not used, the analyses and conclusions drawn are incomplete and possibly flawed. Because of these omissions, interpretation of data available to CASAC and OAQPS may lead to misinterpretation of the health effects. Our Technical Support Division is providing California PM <sub>2.5</sub> and PM <sub>10</sub> data for you. A copy of the disk and information is enclosed herewith.	We thank CARB for providing additional data. Their data, along with other daily data, has been used to prepare a new section, 6.10, Fine and Coarse Particulate Matter Trends and Patterns (45 pages), which gives information based on daily as well as seasonal data. The differences between California and Philadelphia are made evident in terms of PM <sub>2.5</sub> and PM <sub>10</sub> daily and seasonal variation and relationships.	Wilson
IIAEC-051	Letvin/ Kennecott	Chapter 6	051-6-1	The final CD should give information on the highest, second highest, and 95th percentile 24-h average concentrations observed in the various regions, as well as trends in these statistics.	A new section, 6.10, (45 pages) has been added that includes some information on highest, second highest, and 95th percentile data.	Wilson
		Chapter 6	051-6-2	Scales should be provided for Figures 6-19a, 6-20a, 6-21a, 6-24, 6-30a, 6-34a, 6-38a, 6-42a, 6-46a, 6-50a, and 6-54a. These figures should not be used merely to show the monitor locations; they should also show concentrations.	Contractor was unable to supply this information without completely redoing each figure. This was not feasible due to time restraints. Relative concentrations can be inferred from the circles on the figures.	Wilson
IIAEC-052	Loney/TVA	Chapter 6	052-6-1	Commentor expressed concern that the spatial and temporal coverage offered by IMPROVE and NESCAUM networks is not adequate to define "aerosol regions" which the staff paper generated based on the maps in Chapter 6.	The inadequacy of the data was noted in the CD. However, it may still be useful to think in terms of aerosol regions even if we lack adequate data to be sure how homogenous the regions are.	

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IIAEC046	Barnes	Section 7.2.5.2.2	046-7-1	Stress any IgE formation has potential for producing hypersensitivity.	Noted.	Mage
		Section 7.2.5.3.3	046-7-2	Add a reference for latex particles in ambient air.	Noted, but suggested reference was not added due to an oversight.	Mage
		Section 7.2.5.8	046-7-3	Place more attention on sampling and analyses of spores and expand discussion. Increase coverage of sample collection methods.	Noted. Because indoor allergens are not correlated with ambient PM concentrations, they can be ignored in the context of health effects correlated with ambient PM concentrations. Because of space requirements it was not deemed of sufficient importance to expand on space sampling/analysis.	Mage
				046-7-4	Overall: Section weights visual methods over modern immunological techniques.	Mage
IIAEC050	Antonsen/ UARG	050-7-1	I. B. 1. If individuals are not encountering PM of quality measured at ambient station it is wrong to conclude that this ambient PM is responsible for the noted health effects.	See Appendix D. The critical item is the exposure of people to PM of ambient origin. Therefore the use of an ambient monitor is not an "ecological" limitation since people are exposed to this ambient PM at home as well as outdoors. The indoor PM that is generated independently cannot be a confounder since it is not correlated with the ambient PM.	Mage	
		050-7-2	I. B. 2. Janssen (1995) is an abstract and the results are unpublished.	Agreed. Janssen et al. (1995) is only discussed on the basis of the information in the published abstract.	Mage	

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IAR052	Antonsen/ UARG		050-7-3	I. B. 3. The DCD and DSP should quantify exposure to PM in the ambient environment.	See Appendix D, Figure 1. People are exposed to ambient PM outdoors and also indoors as PM readily infiltrates into indoor micro-environments. It is the total exposure to PM of ambient origin, both indoors and outdoors, that is important.	Mage
					Disagree. It is clear that if there is a linear relationship between ambient PM and community health, as assumed by the epidemiological models, then the higher the community mean, the higher the rate of health impact on the community. The ambient monitor is related to the average exposure of the population in that community to PM of ambient origin. See Appendix D to Response to Comments on PM NAAQS Proposal.	Mage
IIAEC052	Loney	Page 7-93	052-7-1	What are the eight other sites mentioned in lines 1-3?	See Appendix D. The relationship between total exposure to sulfate and ambient sulfate data clearly establishes the relationship between exposure to PM of ambient origin and ambient PM concentration. The comment does not distinguish between personal exposure to indoor and outdoor PM and personal exposure to only PM of ambient origin ("Individual personal monitors correlate poorly with ambient monitors").	Mage
					The ones mentioned in the preceding paragraph, e.g., The six 6-City sites and the two NYC ERDA sites.	Wallace
IIAEC054	Keller/Klimisch		054-7-1	Roth-Enclosure 1. Also handouts to CASAC of 12/11/95. "Outdoor ambient PM levels are not correlated with, and are poor surrogates for, personal exposures to PM".	Outdoor ambient PM levels are correlated with, and are good surrogates for, personal exposures to PM of ambient origin. See Appendix D to Response to Comments on PM NAAQS Proposal.	Mage

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Docket #	Commentor	Line/Tab/Fig	Ch-Pg Com #	Comment Summary	Response	Responder(s)
	Keller/ Klimisch		054-7-2	Deleting subjects with ETS exposure eliminates an important real-world source of exposure.	Disagree. ETS is uncorrelated with ambient PM. Mage Therefore adding an uncorrelated variate to the ambient PM mass collected by a PEM must cause the correlation between ambient PM and exposure to ambient PM, as measured by the mass on the filter contaminated with non-ambient PM, to be degraded. Furthermore, health effects of ETS must be independent since ETS does not fluctuate with the ambient PM data. Figure 7-30 (attachment II-38) is misinterpreted. It shows how including people with ETS exposure in the cross sectional studies can destroy the underlying longitudinal correlation for the individuals both with and without constant ETS exposure. The reviewer does not address the argument raised by EPA that ETS cannot be a factor in these studies since it is independent (like a random weighing error of the filter) of the ambient PM. See also Appendix D.	
			054-7-3	Figures II-20 to II-27 show no association of outdoor levels of PM and personal exposures of PM10.	The 7 models all include indoor and outdoor PM. Because 2/3 of the indoor PM in Riverside can be ascribed to ambient sources, the models are not separating out independent effects of PM from ambient sources and PM from purely indoor sources. See Appendix D discussions.	Mage

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Docket #	Commentor	Ch-Pg		Comment Summary	Response	Responder(s)
		Line/Tab/Fig	Com #			
	Keller/ Klimisch		054-7-4	Using outdoor data for personal exposures in an epi study can generate false positive values even if there exists a positive correlation between outdoor levels of PM and personal exposure to PM.	The hypothetical data set presented here is based on an implicit assumption that there is in fact no health effect (response) related to PM, because it is not correlated significantly with total personal exposure to PM, so the correlation with ambient PM must be spurious. Because personal PM exposure includes exposure to PM of ambient origin plus indoor generated PM we can estimate the indoor generated PM as shown on Table 1. Assuming the subjects are exposed to 2/3 of the SAM while indoors and spend 10% of their time outdoors, the exposure to non-ambient PM is PEM - 0.7 SAM, so we can calculate the non-ambient values as shown. These non-ambient values have a negative correlation with the response (-0.25). Assuming the non-ambient or indoor generated PM is relatively inert and the ambient PM is the highly potent species, this analysis shows how using personal exposures to the total PM (which consists of active ambient agents and passive indoor agents) can mask a significant correlation of $r = 0.64$ , with a $p = 0.002$ . Another interpretation here is that a significant association between personal exposure to PM of ambient origin and responses becomes insignificant when personal exposures to the total of ambient PM plus indoor PM are used as surrogate measures of personal exposures to PM of only ambient origin.	Mage

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	Keller/ Kimisch		054-7-5	Valberg: Enclosure 2. One must ask "Why are there day-today fluctuations in [ambient] PM?", and, "Will not the same factors that cause such fluctuations in [ambient] PM also signal changes in human behavior and exposure, which may be causally linked to morbidity and mortality?"	No! The reviewer cites the Eastern Washington Dust storms as producing "a lack of respiratory problems". If the reviewer's hypothesis is correct, and those individuals who go indoors during the dust storms have higher exposure to indoor PM, shouldn't they have respiratory problems from the increased indoor air pollution they would suffer during a dust storm from spending so much more time indoors? See also Appendix D of Response to Comments on PM NAAQS Proposal.	Mage
			054-7-6	Do the ambient PM measures, derived from stationary central monitors in the epidemiologic studies, accurately reflect personal exposures?	Yes, indeed, the ambient PM measures, derived from stationary central monitors in the epidemiologic studies, accurately reflect personal exposures to PM of ambient origin. See Appendix D of Response to Comments on PM NAAQS Proposal.	Mage
			054-7-7	Individual [total] exposure is much more relevant than community mean exposure [to ambient PM].	No. See response to Roth below and Appendix D. The presence of indoor sources has no influence on the equilibrium amount of ambient PM found in an indoor microenvironment.	Mage
			054-7-8	Enclosure 4: Valberg 12/11/95 Climate stress promotes time indoors and usage of climate control systems that increase the levels of potentially toxic PM indoors.	Disagree. See Appendix D to Response to Comments on PM NAAQS Proposal.	Mage
			054-7-9	Failure to include ETS as part of the exposure may distort magnitude of respiratory effects ascribed to ambient PM.	Disagree. People do not smoke more on days of high pollution than on days of low pollution. See Appendix D.	Mage
IIAEC055	Roth Associates		055-7-1	The importance of indoor air cannot be stressed enough.	Disagree. Indoor air is a <i>non-sequitur</i> in the study of ambient PM effects because sources indoors are uncorrelated with ambient PM. See Appendix D.	Mage



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Roth Associates						
055-7-2				It is inappropriate to remove outliers to improve correlation.	The data are analyzed both ways, with and without the outliers, so the reader can see what the effect is of non-ambient pollution sources and how they mask the underlying relationship between exposure to PM of ambient origin and the ambient concentration. It makes sense to remove the high non-ambient exposures created by identifiable rare indoor sources.	Mage
055-7-3				Enclosure 5: Outdoor monitors cannot be used as an accurate representation of personal exposure.	Disagree: See Appendix D. Outdoor monitors can be used as an accurate representation of personal exposure to PM of ambient origin..	Mage
055-7-4				The importance of indoor sources and the "personal cloud" effect has not been adequately addressed.	Disagree. They are not important. See Appendix D.	Mage
055-7-4				Outdoor PM monitors do not correlate with personal PM exposure.	This is conceptually incorrect. The important truth is that outdoor PM monitors do not correlate with personal exposure to <u>PM of ambient origin</u> .	Mage
055-7-5				Bahadori et al. (1995) and Rojas-Brancho (1996) present data on COPD patients exposure to PM which shows no significant correlation between PEM and SAM.	These unpublished data are very interesting to should be compared to the Janssen et al. unpublished results in non-A/C homes. As discussed in Appendix D, the presence of A/C in 9 of the ten homes is a confounder here. First, there is a filter in the return air to the A/C unit which removes some PM and the hi-velocity inlet air suspends previously settled PM, as discussed by Valberg (see AAMA comments #054). This may explain the large personal cloud observed for these COPD patients, which would obfuscate the underlying positive relationship that must exist for PM of ambient origin and produce the negative r values cited: i.e., given a home with a fixed air exchange rate and an A/C system in continuous operation,	Mage

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	Roth Associates		055-7-5		Response (cont'd)	Mage
					when the ambient PM increases, the PM of ambient origin circulating in the home must also increase. Also see the Suh et al. sulfate data in Appendix D which has a good correlation in A/C homes. If the unpublished work cited by commentor is submitted for publication and peer review, it should also report the correlation between personal sulfate exposure and simultaneous ambient sulfate data using the PM <sub>2.5</sub> fraction.	
IIAEC056	Leonard		056-7-1	Do not rely on unpublished studies.	Agreed. Janssen et al. is removed, but Tamura et al. (which is published) is added.	Mage
			056-7-2	One has to consider all the PM that one is exposed to.	Disagree. See Appendix D. Indoor sources of PM are uncorrelated with ambient PM; so, indoor PM cannot produce the health effects associated with ambient PM fluctuations.	Mage
			056-7-3	Highlight the new Bahadori et al. study - in preparation.	The study can not yet be relied upon because it was unpublished.	Mage
		Page 7-4	056-7-4	Consider smoker's exposures as well as non-smoker's exposures.	Gradient Corp. Comments were cited as an appropriate elaboration. See response to Valberg in the AAMA response above.	Mage
		Page 7-12	056-7-5	If generic PM is causing health effects, any PM will have an elevated risk.	Disagree. All particles may not have identical toxicity. See Gradient Corp response (op. cit.) which acknowledges that dust storm PM at 1 mg/m <sup>3</sup> does not cause respiratory distress. Because of the independence of indoor generated PM from PM of ambient origin there can be no confounding due to the indoor PM health effects, if any.	Mage

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Docket #	Commentor	Line/Tab/Fig	Ch-Pg	Com #	Comment Summary	Response	Responder(s)
Leonard		Page 7-93	056-7-6		Phillipsburg is atypical. Tokyo is atypical.	Disagree. The Tokyo situation proves the rule. In the absence of indoor sources, there is excellent correlation between PEM and SAM. The laws of diffusion and mass balances are the same in Japan as they are in the USA; so the same relationship that personal PM exposures of elderly people are related to ambient PM in the absence of indoor sources holds also in the USA. If the indoor sources add PM to the personal exposure, but are uncorrelated with the ambient PM, then of course the correlation between personal exposure and ambient will be driven down by the noise of the indoor PM emissions. See Appendix D. Phillipsburg may be atypical but the result is not counterintuitive. A point source will lead to higher PM exposures downwind than upwind in the community. Thus averaging the PM exposures will regress towards a mean no matter which direction the wind is from if the people sampled live surrounding the plant.	Mage
		Page 7-94.	056-7-7		The unknown indoor sources of PM need elaboration.	Disagree. See Appendix D. Indoor generated PM is uncorrelated and independent of the ambient PM (people do not smoke more on high pollution days than on low pollution days). Therefore any health effects associated with indoor PM must also be independent of and uncorrelated with those health effects produced by the ambient PM people are exposed to, both indoors and outdoors.	Mage
		Page 7-195 to 198.	056-7-8		This whole section is filled with speculation.	The "speculation" is backed up by analyses of the PTEAM data which show that indoor generated PM appears to be uncorrelated with ambient PM concentration, which would explain the low correlation when strong indoor sources are present. See Appendix D.	Mage

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Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
Leonard		Page 7-208	056-7-9	Conclusion 18 is too strong.	Disagree. The data of Tamura et al., 1996, backed up the Janssen study (which was removed) and the Lioy et al. Phillipsburg study. See Appendix D for more discussion.	Mage
		Page 7-209	056-7-10	Conclusion 19 is too vague.	Agreed. It was replaced with new conclusion (5) that was strengthened by citing sulfate as a case-in-point.	Mage
			056-7-11	Chapter 7 omits discussion of occupational exposures in industry and the "dusty trades". This is unfortunate.	Disagree. Occupational exposures are not a common mix of PM chemicals such as found in the soup of urban smog, so that all people in the community are exposed to the same approximate mix of chemicals. Workers in textile mills are exposed to cotton dust, workers in coal mines are exposed to coal dust, and workers in non-ferrous smelters are exposed to manganese dust. It is beyond the scope of this CD to look at each specific PM species which may have a different toxicity as expressed by different endpoints as a function of the specific toxicity. Furthermore, occupational exposures are not correlated with ambient PM, so the health effects they create are independent of and uncorrelated with the health effects associated with fluctuations of ambient PM.	Mage
IIAEC058 Wyzga		Page 7-4, line 13-15	058-7-1	Add Yoshimura.	Agreed. Yoshimura is added.	Mage
		Page 7-9, lines 13-21	058-7-2	Confusion over word coarse.	Disagree. It is defined earlier in the text.	Mage
		Page 7-28+, 7-29	058-7-3	There is a difference between a central ambient monitor and a back yard SAM.	Agreed. Corrections made.	Mage
		Page 7-35	058-7-4	Where are the results for the PM <sub>2.5</sub> .	PTEAM pilot did not measure personal PM <sub>2.5</sub> .	Mage
		Page 7-73 and 7-202	058-7-5	Need to address the resuspension problem.	Noted.	Mage

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	Wynga	Page 7-79	058-7-6	Are there any hospital exposure data to add?	No. None available for patient personal exposure.	Mage
		Page 7-103, line 15	058-7-7	Give size distribution figures.	Agreed. It was < 10 um AD.	Mage
		Page 7-124, Table 7-25	058-7-8	Add correlations were not significant for all individuals in the study.	Agreed.	Mage
		Page 7-140, Table 7-32	058-7-9	The low R <sup>2</sup> should be specially noted.	Disagree. This is a small pre-pilot study with developmental equipment and small numbers of observations. The full-scale pilot study with some 179 subjects is more meaningful.	Mage
		Page 7-162, Figure 7-38,	058-7-10	Where is the personal cloud.	It is in Figure 7-38 d.	Mage
		Page 7-166 to 7-193.	058-7-11	Individuals die, not communities.	True, but total numbers of individuals that die on a given day in a community can be related to average ambient PM exposures.	Mage
		Page 205, line 27-34	058-7-12	Can't make sweeping statement because of variability among individuals.	Agreed. Caveat is added.	Mage
		Page 198	058-7-13	The significance of average personal exposure is not meaningful.	Disagree. See Appendix D to Response to Comments on PM NAAQS Proposal.	Mage
		Page 205, lines 17-20.	058-7-14	The real conundrum is epi finds relations in cities where PEM and SAM do not correlate.	The crucial correlation is that between personal exposure to PM of ambient origin, and ambient PM concentrations. When indoor sources are operating in an independent and uncorrelated manner they cause the correlation of personal exposure to ambient plus non-ambient PM with respect to ambient PM to go down precipitously. See Appendix D to Response to Comments on PM NAAQS Proposal.	Mage
		Page 206 line 5,6	058-7-15	Replace are by can or possibly.	Agreed, change made.	Mage

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Wyzga		Page 206, line 16	058-7-16	"But not for the exposure of specific individuals".	Agreed. Changed "surrogate for personal exposure" to "surrogate for average personal exposure".	Mage
		Page 208, line 1-4	058-7-17	This conclusion is premature.	Agreed. Bahadori is not published yet. COPD is Mage not mentioned in final draft to cover this.	
		Page 207, 9-12	058-7-18	Confusing. Distinguish between epi study types.	Agreed. Correction made.	Mage
			058-7-19	Comments on the following Pages: 7-58 and 7-59, 7-93, 7-95, 7-101, 7-125, 7-130, and 7-132	Subject pages deleted to meet the need to shorten Chapter 7, as recommended by CASAC.	Mage
			058-7-20	Comments on the following Pages 7-4, 121-22, 7-5, 7-147 all 4 comments, 7-161, 7-166, 7-194, 7-195, 7-196, 7-197 7-200, and 7-208	These comments are noted, but do not require any formal response.	Mage
			058-7-21	Comment 1: SAMs introduces error in estimating exposures.	See Appendix D. The SAM is a good estimator for exposure to PM of ambient origin.	Mage
			058-7-22	Comment 2: The best PEM/SAM associations were for Dutch children.	This implied that the associations are best for individuals who are not smoke exposed. However, this section was deleted because these Dutch data were not yet accepted to be published in a peer-reviewed journal.	Mage
			058-7-23	Comment 3: SAM and PEM correlate better at high ambient PM concentration.	Agreed. This is mentioned in the text. Indoor sources make a smaller contribution and have less impact on the correlation coefficient when the ambient PM is high.	Mage
			058-7-24	Comment 4: The implications of the above needs review for different epi study designs.	That belongs in the epi chapter where the different epi study designs are discussed.	Mage
IIAEC-057 Hill/ Appalachian Mountain Club		Chapter 8	057-8-1	Submittal of research report (research conducted by Appalachian Mountain Club) for inclusion in the visibility chapter.	EPA notes submission of the research report; however, because of policy not to use non-peer review studies in the document, we are unable to use the information at this time. EPA notes that an journal article discussing the findings of the study is to be peer reviewed.	Comfort

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		Ch-Pg			Response	Responder(s)
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IIAEC-053	Ammann/ Olympia, WA	p. 12-97	053-12-1	This discussion of asthma is inadequate.	This discussion of asthma is meant only as a very brief introduction. It's content developed in response to CASAC members input.	Kotchmar
			053-12-2	Conversions and calculations are made by the CD author but presented as though they are from the original author. These hidden calculations misrepresents the studies	See pages 12-14 to 12-15. Providing such a uniform comparison basis was considered to be most appropriate by the vast majority of reviewers	Kotchmar
IIAEC-053	Ammann	p. 12-100, line 7,8	053-12-3	Suggest "approximately 300/community".	Not an improvement, not implemented.	Kotchmar
		p. 12-100, line 9	053-12-4	Suggest "in each community" and "hourly and daily..."	Not an improvement, not implemented.	Kotchmar
		p. 12-100, line 16	053-12-5	Suggest "averaged for the six cities".	Not an improvement, not implemented.	Kotchmar
		p. 12-100, lines 19 and 20	053-12-5	Statement "cough was significantly related to all pollutants except acidity" is not correct. Suggest other text for other OR info.	Deleted the sentence questioned. Remainder of text appropriate	Kotchmar
		p. 12-100, line 22, line 27, and lines 30 and 31.	053-12-6	Suggested correction to odds ratio shown. What is meant by "these curves show an inconsistent relationship at lower exposures".	Text odds ratio derived directly from table in publication. For SO <sub>2</sub> and H <sup>+</sup> examination of the curves at lower exposures in Figures 12-3 and 12-4 show a line the moves up and down and is "not consistent" as compared to "higher" exposures where a straight line is observed.	Kotchmar
		p. 12-102, line 10	053-12-7	The average during the study period needs to be cited. Several other analysis need to be mentioned. SO <sub>2</sub> was not monitored during the study period. Define limited.	The average of 46 µg/m <sup>3</sup> has been added. Other analysis as appropriate are discussed in Section 12.6 of the chapter. Limited include none and study period monitoring. The concept is that limited direct information is available other than historical information showing reduced levels.	Kotchmar
		p. 12-103, lines 1 and 2	053-12-8	Here is an example of calculated values by the author of the section. There is no way that the reader can be aware that these numbers were not reported in the study.	As mentioned in response to public comment 053-12-2, all studies were presented in a uniform outcome measure to make interstudy comparison possible. These conversions are not interpretations and are meant to help the reader and make the overall process easier. As in most other/all reviewers appreciated/asked for this approach.	Kotchmar

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IIAEC-053 (cont'd)	Ammann	p. 12-103, lines 1 and 2	053-12-9	Suggests "patients" "receiving treatment by local physicians". The same air quality data does not need to be restated here. It was in the above text. Incorrect statement made.	Description of asthmatics appropriate as presented. Air Quality data not repeated as suggested. "Incorrect" statements are correct thus not changed.	Kotchmar
		lines 3 to 5, lines 8 to 10				
		p. 12-103, lines 11 to 14, line 17, lines 19 to 24, line 24	053-12-10	A more detailed description of the study is suggested. PM values are incorrect. No SO <sub>2</sub> monitoring was done. Important results are left out. Where does minimum temperature information come from. The study only reports mean daily low temperature.	The discussion is appropriate. Values corrected. Thank you. Clarified to say no SO <sub>2</sub> monitoring. All results can not be reported here. The appropriate results are provided. If each study was increased as suggested, the length of the document would substantially increase. The study is described appropriately. Low daily temperatures were reported as the comment notes.	Kotchmar
		p. 12-104, lines 5 and 6	053-12-11	Are these calculations presented in the paper or created by the CD.	These outcomes are reported in the paper as published. See Ostro et al. (1995).	Kotchmar
		p. 12-104	053-12-12	Suggestions are made for additional text for the Schwartz et al. (1991) study. Also specific corrections are identified.	While expansive clarifying of most papers would be informative, this study is appropriately described. Changed number of monitoring to read "one to four". Corrected spelling of the city Köln and Freudenstadt.	Kotchmar
		p. 12-104	053-12-13	Suggestions are made for additional text for Hoek and Brunekeef (1993) and specific correction are noted.	Proximal deleted. Instrument changed to inlet designed. Appropriate limited changes implemented.	Kotchmar
		p. 12-105	053-12-14	Suggestions are made for changes to the text description of Hoek and Brunekeef (1994)	Pulmonary function and respiratory systems inserted	Kotchmar
		p. 12-106	053-12-15	Suggestions/corrections for Roemer et al. (1993)	313 corrected to 131. Instrument changed to inlet design. Similar to seasonal variation sentenced deleted.	Kotchmar
		p. 12-108, line 17	053-12-16	Study conclusion or CD author.	Study.	Kotchmar
		p. 12-108-109 line 29	053-12-17	Sentence not clear.	No change.	Kotchmar
		p. 12-112 thru p. 12-117	053-12-18	Calculations are not shown.	See response to comments 053-12-2, 053-12-8.	Kotchmar
IIAEC-054 AAMA	Keller/Klimisch General			The AAMA submits review from consultants. Some blending of comment on the CD and staff paper occurs. Some non specific statements are made.	The individual reviews are responded to below. The focus here relates to specific comments on the CD only.	Kotchmar

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# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Ch-Pg	Commentor	Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
AAMA/Roth Associates				054-7-1	Roth Associates present analyses of several studies of PM exposure that are in Chapter 7 of the CD. They present their results and conclusions. No specific changes are suggested to the CD. The effort is aimed more at the staff paper.	The exposure discussion for PM is important. While this comment/discussion can not be directly responded too, Chapter 7 has been revised and in response to other comments and updated information and analysis included that provides an appropriate review of the area.	Kotchmar Mage
				054-12-1	Roth Associates present analysis and discussion of PM mortality studies which are the same studies that have been reported in the literature and compare and contrast the results. No direct CD comment.	The CD actively reviews various studies examining analysis of PM mortality in the same cities. Philadelphia being the main example, HEI analysis included. This aspect was updated and revised based on other specific comment.	Kotchmar
				054-12-2	Roth Associates present their assessment of the fine particle epidemiological data. They do not refer to the CD.	Direct reference is made to the staff paper. Review and revision of these studies in the CD made based on other comments and new data some in press studies now published.	Kotchmar
AAMA/Gradient Corp.				054-12-3	Roth Associates present comment on the meta-analysis in Chapter 12 and other published meta-analysis.	CASAC review of the meta-analysis and its use in the CD were considered appropriate. See response to comment 005-12-6, 026-12-18, 026-12-19.	Kotchmar
				054-12-4	Gradient Corp. presents a discussion of PM (1) laboratory toxicology, (2) characteristics of ambient PM, and (3) exposure assessment. Provides their view and insight about this data.	The CD is quoted at times supporting their view at other times disagreeing with the conclusion. EPA considers the areas identified as important topics where additional research are needed. The final revisions present conclusions that are considered appropriate.	Kotchmar
AAMA/EHC and other separate consultants				054-12-5	Various issue papers and statements with no direct CD comments.	The CD appropriately discussed the content of these issue papers and statements.	Kotchmar
IIAEC-055 Li/Roth Roth Associates	pp. 12-276 to 277	055-12-1			Response comments to EPA's presentation and review of Li and Roth (1995) is provided.	The PM CD discussion of Li and Roth (1995) considered appropriate as presented. No change implemented.	Marcus
IIAEC-056 Leonard/GM	Chapter 12	056-12-1			A general discussion of key issues with comment both on the staff paper and CD presentation.	These key issues are important areas. Revisions to the CD are responsive. Different viewpoints are noted.	Kotchmar

# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC-058	Wyzga	Chapter 12	058-12-1	General comments note need for more discussion of exposure error, threshold issue, and coherence.	The revised document continues to develop as the data allows discussion on exposure error and thresholds. Non-specific coherence does provide some insights.	Kotchmar
		p. 12-4, lines 5 and 6	058-12-2	Results also should be comprehensive.	Sentence correct as is.	Kotchmar
			058-12-3	Exercise and activity levels also important.	Discussion appropriate to cover this potential consideration.	Kotchmar
		p. 12-13, lines 20 to 26	058-12-4	Correctly specified needs to be underlined.	Not needed. Importance of phrase strong as shown. Discussion appropriate.	Kotchmar
		p. 12-15, lines 24 to 27	058-12-5	It needs to be determined if alternative specification are really very different.	Additional research may provide more information	Kotchmar
		p. 12-16, p. 12-17 p. 12-22, p. 12-24	058-12-6	Various commentary with inspecific remarks of a general nature.	No change appropriate. Some comments interesting.	Kotchmar
		p. 12-26, lines 9 to 18	058-12-7	Lipfert, 1995 summarizes control variables.	Discussion is appropriate as is.	Kotchmar
		p. 12-43, line 12	058-12-8	Only a limited number of studies from the same author is presented. Mention Lipfert and Wyzga (1995).	Text changed to note the Lipfert and Wyzga (1995) report results in terms of elasticities.	Kotchmar
		p. 12-44, line 24	058-12-9	How do we know they were "effectively" modeled? Delete this word.	Sentence correct as is.	Kotchmar
		p. 12-47, line 26	058-12-10	Is Cifuentes and Lave in press.	The current status is noted.	Kotchmar
		p. 12-47	058-12-11	You may want to add Wyzga and Lipfert (1995).	Discussion of Philadelphia TSP studies updated to include further analysis by Samet et al. from HEL. Discussion of other papers made elsewhere as appropriate.	Kotchmar

# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC-058	Wyzga	p. 12-55	058-12-12	The text is confusing. Were 12,055 days estimated from 1387? One would expect greater measurement error for PM <sub>10</sub> than for PM <sub>2.5</sub> , yet they show similar results.	The smaller number is for a specific city. The total sample for all cities was 7,436 days which due to the exposure metric used increased the total to 12,055 days for all cities. While exposure error may be greater for PM <sub>10</sub> vs. PM <sub>2.5</sub> , it may not be to such a magnitude that the results being similar provides or does not provide insight into that question.	Kotchmar
		p. 12-56	058-12-13	This would be a good place to introduce Lipfert and Wyzga (1995). This paper compares elasticities. It is curious that these results are ignored in this chapter.	Lipfert and Wyzga (1995) introduced with mention of elasticities on p. 12-48.	Kotchmar
		p. 12-57, p. 12-58	058-12-14	Li and Roth (1995) from Particulate Matter: Health and Regulatory Issues. A WMA analyzed Birmingham and Toronto. Threshold should be mentioned.	This study was reviewed by EPA and discussion of it was not deemed appropriate. CASAC comments are supportive of this approach in view of CASAC closure on this chapter at that time. Additional discussion did not significantly extend the issues discussed.	Kotchmar Marcus
			058-12-15	What are the pollution levels in Santiago?	The mean and maximum PM <sub>10</sub> levels are shown in Table 12-2 which tend to be higher than in the U.S.	Kotchmar
		p. 12-63 to 12-65 Tables, 12-3 and 4 p. 12-66	058-12-16	These tables ignore studies.	Li and Roth, and Lipfert and Wyzga are not included in these tables. Tables are revised to focus on PM <sub>10</sub> in the US and Canada. These tables are considered to be appropriate as is the discussion related to them.	Kotchmar
		p. 12-70	058-12-17	High levels at San Paulo should be mentioned here.	Discussion of study appropriate.	Kotchmar
		p. 12-70	058-12-18	Effects of measurement error could be significant and should be discussed.	General discussion early in the chapter is appropriate. Further discussion by paper appropriate if data available.	Kotchmar
		p. 12-72, lines 4 to 25	058-12-19	Mention Lipfert and Wyzga (1995) alternative particulate measures here.	This paper appropriately discussed earlier on p. 12-48.	Kotchmar
		p. 12-96, line 15	058-12-20	Lengthen the discussion about lag differences.	Length appropriate	Kotchmar
		p. 12-139, lines 25 +	058-12-21	Discuss Lipfert (1995) here.	Revisions to this section tend to reduce the discussion not expand it.	Kotchmar

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# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC-058	Wyzega					
		p. 12-162	058-12-22	Measurement error could be a factor here.	Discussion revised in this section with minor change. No discussion of measurement error beyond chapter discussion in place.	Kotchmar
		p. 12-165, line 34	058-12-23	Where?	See Section 12.4.1.4.	Kotchmar
		p. 12-197, line 6	058-12-24	Discuss Schwartz (1989).	This is a study of children not adults. Study used in discussion on p. 12-242.	Kotchmar
		p. 12-221, line 27-28	058-12-25	This is an overstatement, many camp studies are negative.	Sentence conveys an appropriate summary of the data.	Kotchmar
		p. 12-223, lines 21 to 24	058-12-26	Can White Plain data be used for NYC. Seasonably is not adjusted for by using sine and cosine.	The text discusses this very question. Seasonality adjustment is appropriate.	Kotchmar
		p. 12-226	058-12-27	What about 1989 data.	The strongest relationship was seen with the 1988 summer.	Kotchmar
		p. 12-231, line 20	058-12-28	Cite the Schwartz et al. (1995) paper here.	Detailed discussion of this paper and discussion of the acid aerosol aspects is presented in the main PM text in Section 12.3.1.1.	Kotchmar
		p. 12-235, line 20	058-12-29	This is contradicted by p. 12-209, lines 6 and 7.	Discussion on both pages correct as written. Appropriate caveats and related discussion. Difference between general discussion and specific discussion for specified studies/cities.	Kotchmar
		p. 12-239, line 7	058-12-30	Other criteria may not come to the same conclusion.	This is possible.	Kotchmar
		p. 12-245, line 20	058-12-31	Or to exposure measurement error.	Always a factor.	Kotchmar
		p. 12-246, line 19	058-12-32	Contrast to ambient levels.	Value in study noted.	Kotchmar
		p. 12-247, line 30	058-12-33	This is wrong. When data seasonality adjusted, the association went away.	We disagree with this interpretation.	Kotchmar
		p. 12-248, line 1-3				
		p. 12-248, lines 10, 16 to 18, 23	058-12-34	Exposure error could be a factor. Does London type exposures exist in the contemporary US? Delete even.	Exposure error could be a factor. Specific discussion here not appropriate. A research need to be evaluated concerning London exposures. "Even" is appropriate.	Kotchmar

# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC-058	Wyzga	p. 12-251, lines 7 to 9, lines 17 and 18, p. 12-252, lines 9 to 26	058-12-35	Appear to contradict each other.	The discussion is not contradictory. The discussion is critical, analytical, explorative and hypothetical. Your comment on p. 12-252 is supportive of this.	Kotchmar
		p. 12-253, lines 15 and 16, lines 27 and 28	058-12-36	How? Compare two Philadelphia time periods.	The rest of the paragraph details how. Comparison to other cities was desired.	Kotchmar
		p. 12-262, 12-266, 12-267, 12-277	058-12-37	Harvesting suggested.	Harvesting discussion developed to the extent that the data allows.	Kotchmar
		p. 12-280	058-12-38	Good discussion	Discussion appropriate in limited sense. It is a discussion not a reporting of data.	Kotchmar
		p. 12-282	058-12-39	How is best defined?	Yes.	Kotchmar
		p. 12-285	058-12-40	Discuss measurement error.	Discussion summary appropriate.	Kotchmar
		p. 12-286, lines 28 to 30	058-12-41	This is not always true. Some exceptions are noted.	Minor revision implemented to clarify this important statement.	Kotchmar
		p. 12-288, line 11, line 18, line 29	058-12-42	Replace "is confirmed by" with "is consistent with", "can be" but often not.	Replacement "generally supported by".	Kotchmar
		p. 12-289, line 3	058-12-43	There are cases when it is not a good indicator.	Discussion in text is appropriate. Caveats on expressed limits are noted.	Kotchmar
		p. 12-290, line 27	058-12-44	But personal experience can be much higher.	The indicator of interest here is the ambient monitor.	Kotchmar
		p. 290, lines 16 to 22, 28 to 32	058-12-45	Good.	Positive comments noted.	Kotchmar
		p. 12-291	058-12-46	Discuss measurement error and threshold estimation.	Discussion of threshold as complete as data and appropriate remarks allow.	Kotchmar
		p. 12-295, lines 9 to 11	058-12-47	We are looking for longer term effects.	Discussion text appropriate as is.	Kotchmar

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# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC-058	Wyzga	p. 12-294, line 24	058-12-48	This is not necessarily the case.	Revision to this section has deleted this discussion.	Kotchmar
		p. 12-295	058-12-49	General comment on mortality displacement.	Comment noted.	Kotchmar
		p. 12-299, lines 17 to 20, 30 and 31	058-12-50	Weren't levels of CO out of compliance? Discussion ok if same confounder and mechanism are operative.	May not be an issue for respiratory infection endpoints. Discussion appropriate for situation discussed.	Kotchmar
		p. 12-303, lines 10 to 15	058-12-51	Good.	Positive comment related to exposure measurement error discussion noted.	Kotchmar
		p. 12-318, lines 10 to 12, 19	058-12-52	Measurement may play a role. All, not most.	Discussion appropriate. Most is a better word choice.	Kotchmar
		p. 12-315	058-12-53	Link to Chapter 7.	Discussion is separate.	Kotchmar
		p. 12-324, lines 17 to 19	058-12-54	This measure can make it difficult to compare TSP-PM <sub>10</sub> differences. I like elasticity.	This provided the most unifying comparison within pollutants, a major concern. Elasticities were not used by "most" researchers.	Kotchmar
		p. 12-326, Fig. 12-42, 12-43	058-12-55	How many co-pollutants?	Refer to study description in earlier section in chapter. Scope of meta-analysis limited. See response to comment 026-12-19.	Kotchmar
		p. 12-329, lines 4 to 8	058-12-56	Some results suggest that TSP may not be a relatively insensitive index.	Associations of health effects with TSP have long been observed.	Kotchmar
		p. 12-329, lines 11 to 13	058-12-57	The nonsmoker study may be more relevant.	Interpretation of this study as presented in the papers is difficult in comparison to others.	Kotchmar
		p. 12-330, Table 12-24	058-12-58	How can we compare these four studies?	The difference is between the two extremes.	Kotchmar
		p. 12-332, lines 8 to 12, lines 19 to 22, lines 28 to 30	058-12-59	Author indicated the average time showing the highest association. A growing body is an overstatement. Measurement error makes it difficult to estimate thresholds.	This discussion is based on more in-depth analyses presented in earlier sections of the chapter for various studies where the database was analyzed for that question and the results were reported. A growing body of evidence is appropriate. The general study threshold statement is appropriate.	Kotchmar
		p. 12-334, lines 12 to 14	058-12-60	Comment regarding mortality in children.	The paragraph is caveated and notes the need for research.	Kotchmar
		p. 12-335, lines 21 to 25	058-12-61	The Abbey study suffers less from smokers and geography consideration.	Text discussion appropriate.	Kotchmar

# RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAEC-058	Wynga	p. 12-343, lines 7 to 9, line 21	058-12-62	Discusses PM <sub>10</sub> -PM <sub>2.5</sub> comparisons. Elasticity should be compared.	Discussion noted. Elasticity not the choice of "most" study researchers.	Kotchnar
IIAEC-050	Hunton and Williams/ UARG	p. 12-364, line 6	058-12-63	Minority not seen.	Some is correct.	Kotchnar
IIAEC-050	Hunton and Williams/ UARG	Chapter 12	050-12-1	The EPA-conducted meta-analysis should not be incorporated into EPA's analysis.	CASAC reviewed this analysis. See also response to public comments 003-12-6, 012-12-4, 012-12-6, 026-12-2, 026-12-18, 026-12-19, 005-12-6,	Kotchnar
IIAEC-050	Hunton and Williams/ UARG	Chapter 13 General	050-13-1	The evidence in the CD lacks coherence.	The revised CD discusses coherence as the data allows at length in a manner that CASAC considered appropriate. The data set as a whole is considered to make a strong statement with appropriate cautions noted. Where there is coherence in the data it is noted.	Kotchnar
			050-13-2	There is no known mechanisms for the effects attributed to particulate matter	A discussion of potential mechanisms are presented as appropriate in Chapters 11 and 13.	Kotchnar
			050-13-3	Other discussion in the comments reference the staff paper and not the CD.	No response possible for CD. See staff paper revisions.	Kotchnar
IIAEC-053	Ammann/ Olympia, WA	p. 13-20	053-13-1	Correct proxies	Discussion deleted in revision.	Kotchnar
		p. 13-33	053-13-2	The statement implies that the toxicologic work on animals is not credible	This was not the intent nor does this read that way to other readers. Sentenced revised.	Kotchnar
		p. 13-40	053-13-3	Change "principle" to principal	Deleted in revisions	Kotchnar
		p. 13-41	053-13-4	A concluding statement about hospitalization studies would be helpful here	This discussion is a conclusion statement	Kotchnar
		p. 13-53	053-13-5	Susceptibility.	Paragraph deleted in revision process.	Kotchnar
		p. 13-73, line 18	053-13-6	Add those with cardiovascular disease.	Added.	Kotchnar
		p. 13-77 thru 13-83	053-13-7	Comments on aspects of the implications of relative risk estimates.	This aspect of Chapter 13 has been revised based on CASAC and public comments and no specific side by side comparison are possible due to deletions and revisions.	Kotchnar

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## RESPONSE TO PUBLIC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Commentor	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response		Responder(s)
IIAEC-058 Wyzga		p. 13-36, lines 18 and 19, lines 28 to 30	058-13-2	One new study not a growing body. An overstatement.	Section revised. More than 20 studies noted.	Statement appropriate.	Kotchmar
		p. 13-77, line 22	058-13-3	No evidence from chamber studies.	Asthma may be exacerbated by air pollutants such as PM is a viable theory.		Kotchmar
		p. 13-77, line 30, pp. 13-78, 13-80, 13-84	058-13-4	The existence of a threshold is impossible to ascertain with the measurement error present.	This aspect of the section revised and updated.	CASAC review consider appropriate.	Kotchmar
		p. 13-85, lines 1 to 4	058-13-5	If we accept number literally these studies still not problem free.	Revised discussion less specific.		Kotchmar



# RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-043	Lippmann	Chapter 1	043-1-1	Several specific comments as presented.	Chapter substantially revised based on comments; mainly shortened to brief, concise bullet format in final CD.	Kotchmar
IIAF-033	Legge	Chapter 1	033-1-1	There needs to be a statement indicating that ecosystems have not been addressed.	Statement placed on p 1-1.	Kotchmar
IIAF-031	Seigneur	page 1-2, line 25	031-1-1	Replace 2 by 3 based on discussion of Chapter 3 that fine particles at high humidities can reach 3 µm in diameter.	Changed to 3 to indicate growth to larger diameter.	Wilson
		page 1-5, lines 18-20	031-1-2	OH chemistry is more complex than suggested here.	Agreed, but purpose of Chapter 1 discussion to tell how OH reacts to produce PM, not to discuss OH formation.	Wilson
		page 1-5, line 12	031-1-3	Mention nighttime formation of nitrate.	Added as recommended.	Wilson
		page 1-6, lines 10-16	031-1-4	Describe aqueous chemistry of sulfate and nitrate formation.	Chapter 1 is limited to summary of most important points. Aqueous chemistry of sulfate is described, but that of nitrate is too complex, uncertain, and unimportant to include here. It is covered in Chapter 3.	Wilson
IIAF-029	Wolff	page 3-184, lines 28-30	029-3-1	This definition (of fine particles being formed from gases) is too narrow because it doesn't include elemental carbon.	While some fine, elemental carbon may be formed in other ways, we think most fine elemental carbon is formed from C <sub>2</sub> molecules formed in combustion. In view of the lack of any evidence for other formation mechanisms, this statement should be acceptable for the Summary.	Wilson
		page 3-185, line 2	029-3-2	It (droplet mode formed by gases dissolving in fog or cloud droplets) can be due to particles dissolving in the droplets as well.	Fog and cloud droplets are formed by activation of particles. Subsequent growth is due primarily to gases dissolving in the droplets due to the higher concentration and greater diffusion velocity of the gases relative to particles remaining in fog or clouds after activation.	Wilson
		page 3-185, line 30	029-3-3	Other metals (i.e., Ni, Cr, Cd, etc.) as well as elemental C have been shown to catalyze the oxidation of SO <sub>2</sub> .	In the summary we choose to include only Fe and Mn, the two most important from the standpoint of concentration and rate. Other catalysts are discussed in 3.3.1.4.	Wilson
		page 3-186, line 3	029-3-4	HSO <sub>4</sub> should be NH <sub>4</sub> HSO <sub>4</sub> .	It is equally correct to say HSO <sub>4</sub> or NH <sub>4</sub> HSO <sub>4</sub> . We choose HSO <sub>4</sub> to emphasize the available H <sup>+</sup> .	Wilson
		6-10, Fig 6-5	029-6-1	Figure 10 lacks scale and time period.	Figure reworked. Time periods and scales added.	Wilson
		6-23, Fig 6-13b	029-6-2	Still uses soot instead of elemental carbon.	Changes made throughout chapter.	Wilson

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## RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-029 (cont'd)	Wolff	6-31, lines 14-26	029-6-3	Review suggests we consider the discussion on background concentration he prepared for the staff paper.	Section on Background completely rewritten, now 6.3.1.7. "Natural background" has been carefully defined. References suggested by Wolfe included and discussed, including problem of defining days with "clean" air based on larger average trajectories.	Wilson
		6-33, Fig 6-17	029-6-4	This figure is unreadable.	Map (unreadable) has been deleted and graph of the number of stations measuring $PM_{10}$ as a function of year enlarged.	Wilson
		6-36, Fig. 6-19a	029-6-5	It needs a scale.	Map is too busy for a scale to be useful. Circles only intended to show locations and give relative concentrations.	Wilson
		6-46, Fig. 6-24	029-6-6	It needs a scale.	Scale added.	Wilson
IIAF-031	Seigneur	Chapter 3, general	031-3-1	Thirty-eight specific comments, mostly editorial in nature.	Corrections made as recommended.	Wilson
		Chapter 3, general	031-3-2	Authors have done an excellent job of revising this chapter.	Thank you.	Wilson
IIAF-038	Pierson	Chapter 3	038-3-1	An annotated copy of the chapter and an insert was provided.	Suggested changes were made as appropriate and the insert added.	Wilson
		Chapter 4	038-4-1	An annotated copy of the chapter and an insert was provided.	Suggested changes were made as appropriate and the insert added.	Ewald
		Chapter 5	038-5-1	Chapter 5 will have to be extensively revised.	Chapter 5 completely rewritten.	Pinto
		Chapter 6	038-6-1	An annotated copy of the chapter and an insert was provided.	All suggested changes were made as appropriate and the insert added.	Wilson
		Chapter 6	038-6-2	New Section 6.9 is quite different from material it replaces.	Section numbering was in error. The new section should have been 6.10. Old 6.9 stays in chapter.	Wilson
IAFF-045	Price	Chapter 3	045-3-1	Chapter 3 is adequate for the standard setting process currently underway.	Comment noted.	Wilson
		Chapter 6	045-6-1	More information is needed on trends in $PM_{10}$ and $PM_{2.5}$ .	Trend data from several data sets is presented and discussed in a new Section 6.10.	Wilson
IIAF-050	Hopke	Chapter 6, general	050-6-1	Thirty-one specific comments or suggestions.	Comments and suggestions implemented as possible and appropriate. Extensive new material added on trends of $PM_{2.5}$ and $PM_{10}$ and on background.	Wilson
IIAF-050	Hopke	Chapter 5	050-5-1	Number of suggested changes.	Changes implemented as appropriate.	Pinto
IIAF-029	Wolff	Chapter 5	029-5-1	Various comments.	Chapter extensively rewritten	Pinto
IIAF-030	Koutrakis	Chapter 5	030-5-1	Various comments.	Chapter extensively rewritten.	Pinto

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# RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-045	Price	Chapter 5		Various comments.	Chapter extensively rewritten.	Pinto
IIAF-029	Wolff	Chapter 7	029-7-1	General: Too many unpublished papers are cited.	The chapter was revised to exclude unpublished papers, such as Janssen, et al. 1995.	Mage
			029-7-2	Too much new science.	The chapter has been reviewed extensively within EPA and without and no negative remarks were made on the technical validity of the missing information principle used. The new science is the best way we know to analyze these data. It is noted that Lippmann commended the Agency for this innovative approach.	Mage
		Page 7-28, line 28	029-7-3	Give information on the sharpness of the cut-point.	It is contained in unpublished lab reports and available on request. This is too detailed for the CD.	Wallace
		Page 7-69, 17-21	029-7-4	Too much emphasis on unpublished TEAM results and Thatcher and Layton.	PTeam results have now been published and the reference is cited. These studies are the most recent and have extensive high quality data on all parameters including air exchange rate which makes an accurate analysis of the penetration rate possible.	Mage
		Page 7-132, lines 4-5	029-7-5	Table 7-25 does not make the point.	Table 7-25 shows good serial correlation for six participants.	Mage
		Page 7-151 Figure 7-35	029-7-6	SAM and CIM (central monitor) are blurred for sulfates.	This analysis has not been done for PTEAM data. EPA believes it is not necessary because recent data from Philadelphia (Burton et al. 1996) cited in the document show that sulfate is uniformly distributed in an urban community where there is little primary emissions of sulfate.	Mage
		Page 7-164, line 20	029-7-7	Anuszewski et al. needs more discussion.	This nephelometric study of PEM and SAM data is more appropriate for studying the relation of human exposure to PM of ambient origin because the PM that enters the home where people are exposed is measured by the SAM. These data are consistent with the observation that for homes with indoor sources correlations are low (0.03, 0.14, 0.20, ...) and where there are minimal indoor sources the correlations are high (0.93, 0.91, 0.90, ...).	Mage

# RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-029 (cont'd)	Wolff	Page 207, lines 9.	029-7-8	Correlations can be better or worse. See previous comment	Noted, see previous response above.	Mage
IIAF-030	Koutrakis	Chapter 7	030-7-1	Place indoor section before personal exposure section.	Done.	Mage
IIAF-035	Samet	Chapter 7	035-7-1	Include research needs in this area of personal exposure and ambient concentration.  SP IV-35 Janssen et al., should not be used unless the information is available for detailed review.	Research needs generally not identified in PM CD, but in separate "Research Needs" workshop and document.  Agreed: The only reference is now to the material in the published abstract.	Mage
IIAF-043	Lippmann	Chapter 7; Page 7-4	043-7-1	SP V-2. Definition of exposure is not the formal EPA definition previously given.  Personal exposure to PM should never be considered a surrogate for ambient PM, especially for smokers and those directly impacted by their ETS.	Noted.  Agreed: Changes made.	Mage
		Page 7-14, line 29; line 30	043-7-2	Change is to was. Delete "has".	Agreed. Changes made.	Mage
		Page 7-93	043-7-3	Not all epi studies are time-series studies. Change text.	Agreed. Changes made.	Mage
		Page 7-166	043-7-4	Reduce material preceding section 7.2.6 by 80%.	Agreed, but only reduced by 50%.	Mage
		Page 7-203	043-7-5	Change Figure 7-47 to 7-46.	Agreed. Change made.	Mage
		Page 7-207, line 1; line 16; line 24	043-7-6	Insert mass before concentration; Insert increment after concentration; Delete "possibly".	Agreed. Changes made.	Mage
		Page 7-208, lines 16-20	043-7-7	How does Conclusion #18 differ from #6?	#18 is regression of personal on outdoor #6 is regression of indoor on outdoor.	Mage

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# RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Ch-Pg				Comment Summary		Response	Responder(s)
Docket #	Member	Line/Tab/Fig	Com #				
IIAF-030	Kourakis	Chapter 8	030-8-1	Chapter should include a discussion of the effect of particles on stratospheric chemistry, since heterogeneous processes are very important to stratospheric ozone chemistry.		Aerosols in the stratosphere have been implicated in the loss of ozone through heterogeneous chemistry involving chlorine compounds. Since SO <sub>2</sub> does not change the stratospheric aerosol burden, the effect of aerosols is not relevant to this discussion.	Comfort
IIAF-046	White	p. 8-15	030-8-2	The discussion on chemical and physical properties of particles is very short and incomplete. The section should be eliminated since this information is already presented in a previous chapter.		Section deleted.	Comfort
		p. 36	030-8-3	It should be mentioned that particles with high acidic content, e.g., sulfuric acid versus ammonium sulfate, are more hygroscopic.		Statement added to chapter indicating that inorganic salts and acids are more hygroscopic than most organic species.	Comfort
		p. 38, lines 8 and 9	030-8-4	When sulfate particles are not completely neutralized, as in the case of ammonium sulfate, they can be in the liquid form for relative humidity well below 80%.		At relative humidities below 80%, ammonium sulfate becomes a dry crystal at equilibrium. In ambient air deliquescence particles frequently exist in a non-equilibrium state, thus containing water even when the relative humidity is below the deliquescence point. Section revised to reflect this information.	Comfort
IIAF-046	White	Chapter 8, p. 9 and 32	046-8-1	There is virtually no discussion of light absorption by particles. There is one sentence on its measurement, one paragraph on the difficulty of calculating it from theory, and one paragraph that very selectively discusses specific absorption by elemental carbon. This is an inadequate treatment of something that: 1) is a major component of extinction in many cities; 2) is a determinant of particles' net effect on atmospheric heating; 3) is the only index of fine-particle concentrations in many of the epidemiological studies that distinguish fine particles from PM <sub>10</sub> . How can OAQPS staff consider basing a new standard on measurements (BS, KM, CoH) that are nowhere documented in either Chapter 4 or 8?		Because visibility is affected by both light absorption and light scattering, the chapter contains a discussion of both of the entities. Particle-related decreases in visual range are the result of light scattering by particles. Light absorption is not a significant factor in particle-related decreases in visual range except for particles containing elemental carbon. The section discussing light absorption, as it relates to gases, has been revised to include additional information.	Comfort

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Docket #	Member	Ch-Pg		Com #	Comment Summary	Response	Responder(s)
		Line/Tab/Fig					
IIAF-046 (cont'd)	White	p. 28 and 12		046-8-2	'Visibility' is repeatedly presented as something ineffectable, too subtle and complex ever to be satisfactorily defined. For example, "a knowledge of the atmospheric optical properties alone . . . is not adequate to predict or characterize the visibility" or "to use the light extinction coefficient, or some parameter calculated from light extinction, as a measure of visibility . . . is, in the general case, incompatible with the definition of visibility in Section 8.1." No firm definition given. Why can't atmospheric visibility be characterized in terms of objective optics?	More satisfactory definition of visibility has been provided.. The definition given is in agreement with that provided by the National Research Council (1993) and historical records based on human observations.	Comfort
				046-8-3	The visibility sections give little indication that theoretically sound and empirically robust relationships do exist in the actual atmosphere between particles and extinction, extinction and visual range, and particles and visual range. Whatever, the unresolved battles over aesthetics are, the simple, brute fact is that we have known for a long time that visibilities decline with increasing particle mass concentrations, and are more sensitive to fine than to coarse particles. These relationships should be made known to OAQPS staff.	Comment noted. The sections of the chapter addressing particle-related visibility effects indicate that the greatest reduction in visibility is caused by fine particles, accumulation mode. Coarse particles have a light scattering efficiency of 5 to 10 times less the efficiency of fine particles.	Comfort
				046-8-4	There is little apparent coordination between the discussions of visibility and radiative forcing. For example, they give parallel treatments of the solar flux, illustrated by independent figures (Figures 8-2 and 8-18) that seem not quite to agree quantitatively.	An attempt was made to better coordinate the discussion in the visibility sections with that of the climate. Figures 8-2 and 8-18 don't quantitatively agree because the backscatter fraction for Figure 8-2 is 0.1 and the Figure 8-18 uses a ground reflectance of 0.2.	Comfort

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-046 (cont'd)	White	046-8-5		The section of the economic valuation is unbalanced in its failure to acknowledge the existence of a substantial and legitimate body of literature that severely criticizes the conceptual foundations underlying all the reviewed work. See, for example, <u>The Economy of the Earth</u> by Mark Sagoff, Cambridge University Press (1988).	Comment noted. The available data on the economic evaluation of pollution on visibility will be further addressed by the Regional Haze Program.	Comfort
		046-8-6		The editorial organization of the visibility sections is chaotic. Concepts are introduced out of order (e.g. Equation 8-1) and included in unrelated sections (e.g. brightness and contrast are discussed under "Measures of Discoloration"). Internal references are unreliable.	Section discussing visibility have been reorganized as recommended.	Comfort
IIAF-049	Middleton	Chapter 8	049-8-1	Add a discussion of the Grand Canyon Visibility Transport Commission (GCVTC) study to the chapter.	Draft reports are not discussed in any great detail in the criteria documents; however, mention was made of the work in progress being conducted by the Commission.	Comfort
		049-8-2		The VARED and DAQM models need to be discussed in the chapter.	A brief description of the DAQM model has been included in the section addressing models. The description of the VARED model appears in an unpublished report and has not been included in the chapter discussion; however, reference to the report has been included in the chapter. See response to 049-1.	Comfort
		049-8-3		The GCVTC study is a more comprehensive analysis of the economic and other effects of potential visibility management alternatives than studies currently cited in the chapter.	See response to 049-8-1.	Comfort
IIAF-034	Mauderly	Chapter 10, page 10-150, Table 10-18	034-10-1	Apparent error in Table 10-18; 0.07 should be 0.7.	The typographical error in Table 10-18 regarding the particle per alveolus was corrected to be 0.7.	Jarabek

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-034 (cont'd)	Mauderly	Chapter 10 Tables 10-38 to 10-43; Figs. 10-56 to 10-64	034-10-2	These tables do not agree with actual experimental data. This discrepancy should be discussed.	<p>A conference call was held on January 11, 1996 to discuss the issue raised about the discrepancy between mouse and rat predictions from the proposed laboratory animal dosimetry model. Concern remained after revisions reflected in the November 1995 interim draft that particle burdens found in Tables 10-38 through 10-43 (pages 10-207 through 10-43) and shown in Figures 10-56 through 10-64 (pages 10-214 through 10-220) did not appear to be consistent with some available experimental data provided by J. Mauderly and R. McClellan as attachments to their December 1995 CASAC review comments. Participants included interested members of CASAC, relevant EPA staff, relevant authors of Chapter 10, and authors of the proposed laboratory animal dosimetry author. The confusion due to differences in expression of deposition fractions, e.g., the correction or not for inhalability, and retained versus deposition fractions were discussed. It was agreed by the group that some of the suggested experimental data were inappropriate for comparison of a model estimating deposition since they actually represent retained dose burdens (e.g., those experimental data on talc and diesel from 6-months exposure duration) and this mitigated concerns to some degree but raised concern about the adequacy of the available data for validation.</p> <p>In response to this conference call, a figure (Figure 10-31 on page 10-112) was added to show the model prediction versus available experimental data in the rat. The analysis done for the November 1995 interim draft showing that separate equations are appropriate for each species was accepted. The discussion of the sources of variability that could explain differences between model predictions and the observed deposition data was provided (Pages 10-117 to 10-118) and was considered appropriate. The group agreed that the application should truncate the range of model</p>	Jarabek predictions to particle diameters ≥ 1.0 µm MMAD.

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-034 (cont'd)	Mauderly	Chapter 10 Tables 10-38 to 10-43; Figs. 10-56 to 10-64			Response cont'd	Jarabek
		Chapter 10, page 10-41, Fig. 10-9	034-10-3	Relabel axis to Fractional Total Deposition.	A discussion of the limitation of the application of the chosen dosimetry models within the context of the lack of data on parameters that influence the variability was added Pages 10-144 to 10-145).	Jarabek
		Chapter 10, page 10B-3, Table 10B-2	034-10-4	Show sources of data.	The y-axis on the Figure (now 10-10) was relabeled.	Jarabek
IIAF-037	McClellan	Chapter 10	037-10-1	Chapter is still deficient in providing a clear exposition on the role of dosimetry to calculate effective dose as an intermediate term linking exposure and response.	Description of how different dose metrics might relate to different health outcomes, e.g., deposition as a metric for "acute" mortality and retained dose as a metric for "chronic" endpoints such as morbidity, was enhanced. Limitations of the mechanistic data on the pathogenesis of the endpoints observed in epidemiologic studies to inform the construction of dose metrics was also highlighted.	Jarabek
		Chapter 10	037-10-2	Strengthen the chapter by inclusion of data that would serve to validate the models (Cuddihy et al., 1979 and Wolff et al., 1987).	These data were considered in discussions and deliberations described in response to comments IIAF-034 and IIAF-088.	Jarabek
IIAF043	Lippmann	Page 10-4, lines 20-21	043-10-1	Particles between 0.1 and 1.0 $\mu$ m diameter are not the "most numerous in the environmental air" the particles $\leq 0.1 \mu$ m are the most numerous.	Sentence reworded as follows: "...are generally the most numerous in the environmental air, with the number concentration of particles tending to increase markedly for smaller particles".	Jarabek
		Page 10-5, lines 12-15	043-10-2	Replace with "aerosols (Hatch and Choate, 1929; Raabe, 1971).	Section is accurate as written. No change.	Jarabek
		Page 10-22, Table 10-2	043-10-3	The source cited (Y.C. Fung, 1990) for this table is incorrect. It first appeared in Lippmann (1970), with credit to its sources (Weibel, Briscoe and Altshuler).	The source where we obtained the table is as cited. We appreciate the commentator's concern that this is not the original source.	Jarabek

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF043 (cont'd)	Lippmann	Page 10-26, line 29	043-10-4	Suggested word change: "negligible [---]" to "low"	Done	Jarabek
		Page 10-28, line 4	043-10-5	Insert a paragraph that indicates "Soldernom" criteria have been officially adopted by the ACGIH, the ISO and the EC.	Section revised extensively and description of the status of international agreement achieved and that between the ISO and the ACGIH is included.	Jarabek
		Page 10-44, Fig. 10-10	043-10-6	Acronym "NOPL" not defined. The caption should at a minimum indicate that NOPL = ET.	Naso-oro-pharyngo-laryngeal (NOPL) introduced in Table 10-1 as equivalent to Extrathoracic region (ET). The definition of NOPL is also provided in the revised caption for the figure.	Jarabek
		Page 10-69, line 1	043-10-7	Suggested word change: "in certain" with "uncertain"	Done.	Jarabek
		Page 10-80, lines 14,15	043-10-8	The study of Horvath et al. (1977) is described as one of humans exposed to ultrafine acid particles. This is not physically possible. At the concentration used, the 0.05 µm acid droplets would have coagulated before inhalation. The interpretation of the data needs to be reconsidered on this basis.	Sentence describing Horvath et al. (1977) study and its interpretation was deleted.	Jarabek
		Page 10-120, lines 27,28	043-10-9	The NCRP model is not "still being developed". It is undergoing final approval.	Sentence reworded: "This model was described in outline by Phalen et al. (1991), and at the time of writing a full report of the model is undergoing final approval by the NCRP".	Jarabek
		Page 10-158, Table 10-19	043-10-10	This table indicates that the mass concentrations of the nuclei mode particles are quite similar to the mass concentration of accumulation mode, and this can not be correct.	Corrections were made in the conversion of the reported sampler mass data to aerodynamic diameters (MMAD) for use in the dosimetry model and this changed the inhaled deposition fraction estimates. This table is now "correct" in that it follows mass deposition accurately from the tri-modal source distributions of particle mass defined in Table 10-C2 of Appendix 10C.	Jarabek
IIAF-036	Utell	Chapter 11, p. 11-18, line 18	036-11-1	Sraw should be Sgaw.	Correction made.	McGrath Folinsbee

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-036 (cont'd)	Urell	p. 11-24, line 21	036-11-2	Raynaud's is misspelled.	Spelling corrected.	McGrath
IIAF-043	Lippmann	Chapter 11, p. 11-1, line 4	043-11-1	Insert phrases to clarify meaning "controlled", "mesothelial", "mass".	Section rewritten and changes made.	McGrath
		p. 11-5, line 11	043-11-2	Change "this section" to "Section 11.2.1".	Section rewritten and changes made.	McGrath
		p. 11-5, line 14	043-11-3	Change "subsequently" to "Section 11.2".	Section rewritten and changes made.	McGrath
		p. 11-5, line 16	043-11-4	Change "The chapter" to "Section 11.2".	Section rewritten and changes made.	McGrath
		p. 11-6, lines 8 and 16	043-11-5	Change and/or insert words "I,e", "eliminate"; "pH"; "I"; "pH".	Section rewritten and changes made.	McGrath
		p. 11-18, lines 36 to 59	043-11-6	Change or insert words "HCl"; "of 7.5 um tracer particles"; "4.2 um"; "in a dose dependent manner" "larynx and"; "(C) and exposure-dependent (t)"; "after"; "Ct exposures".	Section rewritten and changes made.	McGrath
		p. 11-59 to 11-184	043-11-7	Change or insert words "Ct"; "Ct"; "SO <sub>2</sub> alone would be highly unlikely to produce such a deep lung response"; add references; "aggregates"; "mass"; delete "from quartz"; "asbestos"; delete "chemical composition"; "IN...strong associations"; delete "6".	Section rewritten and changes made.	McGrath
IIAF-034	Mauderly	Chapter 11 p. 11-99	034-11-1	Many ultrafine particles are inhaled as aggregates. Deposition is different from true ultrafine. Also, note effects of diesel particles. References suggested.	Discussion has been revised as suggested and appears on pp. 11-97 to 11-98 in final document. Suggested references were included. Specific discussion of diesel exhaust appears on pp. 11-102 through 11-126.	Folinsbee
		p. 11-135, Table 11-18	034-11-2	There are several additional reports on chronic particle exposure in animals that should be included-notably NTP bioassay studies.	We agree the list was not comprehensive. We have added several of the suggested references to the Table and have expanded the discussion. The intent was to provide examples. Tables 11-18 through 11-22 included brief information about chronic PM exposure effects on mortality, lung function, physiology and biomarkers.	Folinsbee
IIAF-043	Lippmann	Chapter 12	043-12-1	Approximately 60 specific corrections to the text were suggested.	These corrections implemented.	Kotchmar

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-043 (cont'd)	Lippmann	pp. 12-139 to 12-152	043-12-2	Only one of these 6 papers by Lipfert is in a peer-reviewed publication.	Section rewritten improving the discussion related to these reports.	Kotchmar
		Appendix 12-4	043-12-A	Correction to the Appendix	The Appendix material was redrafted and inserted into the main text or deleted.	Kotchmar
IIAF-029	Wolff	Chapter 12	029-12-1	Many key references are not in the peer reviewed literature. Additional references are just abstracts.	While no specific reference is mentioned, the key references in final CD have been published in the peer reviewed literature. Abstracts are generally not used.	Kotchmar
		p 12-32, line 18	029-12-3	The epidemiological data do not unambiguously support the contention that PM <sub>2.5</sub> is the cause of excess mortality.	Studies that provided PM <sub>2.5</sub> data, while limited, are presented in the document, evaluated, and appropriate conclusions stated in the CD.	Kotchmar
		p 12-70, lines 18 to 21	029-12-4	The information discussed in this section does not support this conclusion.	This additional reference by the commentator has been added.	Kotchmar
		p 12-138, line 17	029-12-5	The Siegal reference is not in the reference section.	Conclusion statement modified.	Kotchmar
		p 12-138, line 17	029-12-6	Do not agree. There will be an O <sub>3</sub> monitor downwind as well to capture peaks.	This reference has been added to the references.	Kotchmar
		p 12-139, lines 14 to 20	029-12-7	What is the reference for these generalizations.	This statement is qualified as "may". Also, there may not be a downwind monitor in place in all situations with data.	Kotchmar
		p 12-147, line 20	029-12-8	Doesn't implication of iron particles implicate the coarse fraction.	Paragraph deleted.	Kotchmar
		p 12-160-161, Table 12-16, Figure 12-7	029-12-9	It should be noted that the "apparently" linear relationship between fine particles and risk "disappears" if the risk for men and women are plotted separately.	The study referenced does not make specific statements. Iron is found in the fine fraction of PM also.	Kotchmar
		p 12-165	029-12-10	A 1995 AWMA Specialty Conference paper by Lipfert needs to be added here that "shows" that the addition of additional lifestyle factors greatly reduces the PM/mortality relationship in the ACS study and eliminates it in the Six City study.	It is "noted that the apparently linear relationship between fine particles and risk is less linear if plotted separately for men and women, but the confidence intervals then become much wider due to smaller samples".	Kotchmar
					This statement here notes specific life style indicators. The paper by Lipfert and Wyzga (1995a) was added as a reference. For a complete discussion, see response to public comment 017-12-1.	Kotchmar

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-029 (cont'd)	Wolff	p 12-179, lines 21 to 24	029-12-11	We do not know how much of the effect is due to harvesting, thus this statement may not be valid.	Paragraph revised and qualified.	Kotchmar
		p 12-270	029-12-12	Discussion in text does not correspond to model numbers in Figure 12-26.	Descriptive information added to figure to clarify.	Kotchmar
		p 12-296, line 12	029-12-13	These figures, 37% and 87%, are not a range.	Text revised and expanded. This comment is addressed in this revision.	Kotchmar
IIAF-035	Samet	Chapter 12	035-12-1	The coverage of the literature is complete and generally unbiased in my view. There are some particularly strong points of the chapter. For example, the sensitivity analyses conducted by Pope are thoughtful and well presented.	Comment appreciated.	Kotchmar
		pp. 12-6 and 12-7	035-12-2	The issue of confounding is introduced here. Use of the word confounding implies independent effects of the component pollutants in complex mixtures. In fact, the pollutants are likely to act through common mechanisms, and we may be inappropriately using regression models in an attempt to identify independent effects. Measurement error further complicates interpretation of model findings. I suggest setting out a broad framework at this point in the chapter that specifically addresses the complex mixture issues.	A revised discussion on the complex mixture issue is found in this section now as suggested.	Kotchmar
		p. 12-13, last para.	035-12-3	Delete this paragraph.	Deleted.	Kotchmar
		p. 12-16, first para.	035-12-4	Modeling is far less empiric than implied.	In general agree. Some reports may be more empiric.	Kotchmar
		p. 12-22	035-12-5	The discussion of thresholds that begins here never acknowledge the limited statistical power of epidemiological data for distinguishing plausible alternatives at low levels of exposure.	This was added to the text as suggested.	Kotchmar

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-035 (cont'd)	Samet	p. 12-23	035-12-6	A comprehensive review is Thomas et al. (1993).	This was cited as suggested.	Kotchmar
		p. 12-70	035-12-7	There is no data presented that would support the conclusion that children are a susceptible group for mortality and PM exposure.	We agree.	Kotchmar
		p. 12-97	035-12-8	The point of Morgan and Taussing is missed.	Sentence deleted.	Kotchmar
		p. 12-98	035-12-9	Is description of common cold needed?	Sentence deleted.	Kotchmar
		p. 12-177, fourth para.	035-12-10	This material is naive and should be deleted.	Paragraph deleted.	Kotchmar
		p. 12-178, lines 15 to 28	035-12-11	This needs to be rewritten.	Text deleted.	Kotchmar
		p. 12-251, last para.	035-12-12	In fact, it appears findings are not particularly sensitive to methods for data analysis.	Text revised to reflect this.	Kotchmar
		p. 12-291, second para.	035-12-13	Another naive discussion of the concept of thresholds.	The discussion uses the most appropriate references to review the topic.	Kotchmar
		p. 12-294	035-12-14	The caution to researchers here is inappropriate.	Paragraph revised to discuss new information on mortality displacement.	Kotchmar
IIAF-037	McClellan	Chapter 12	37-12-1	Include some relevant background data on health statistics, particularly for cardiovascular and respiratory diseases.	A new Table 12-1 Age-specific United States Death Rates was added. A similar table on hospitalization is already in the chapter.	Kotchmar
			037-12-2	The chapter needs to provide appropriate information on the interpretation of linear regression data especially in regards to thresholds.	This is discussed as appropriate to put this into perspective.	Kotchmar
IIAF-039	Shy	Chapter 12	039-12-1	The revised Chapter 12 adequately addresses many of the concerns raised in the August 31, 1995, letter of the CASAC chair to the EPA Administrator.	Comment appreciated.	Kotchmar
IIAF-040	Ayres	Chapter 12, p. 12 to 27	040-12-1	The high concentration pollution is a natural experiment.	The discussion here and later in Section 12.5 presents this information.	Kotchmar
			040-12-2	Various other specific commentaries.	Statement doesn't require specific changes.	Kotchmar

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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-047	Larntz	Chapter 12	047-12-1	Discussion of regression analysis, especially using copollutants as covariates. Specific general comments other statistical methodologies are useful.	Specific changes implemented as possible.	Kotchmar
IIAF-050	Hopke	Chapter 12	050-12-1	There is a good correlation of health effects with PM <sub>10</sub> . There are limited fine particle data.	The CD presents the available data for both PM <sub>10</sub> and PM <sub>2.5</sub> and provides appropriate conclusions with the appropriate cautionary statements.	Kotchmar
IIAF-062	Speizer	Chapter 12	062-12-1	The chapter is too long. There has been considerable improvement. It represents as complete as compendium of work on the subject as can be imagined.	Comment noted and appreciated.	Kotchmar
		p. 2-47, line 7	062-12-2	A more recent version of the Cifuentes and Lave work should be cited.	The rewritten paper in 1996 is referenced.	Kotchmar
		pp. 12-53 and 12-54	062-12-3	Cite later version of Pope and Kalkstein. Last sentence is meaningless. Many Lipfert publications cited are not peer reviewed.	In press version of paper cited. Sentence rewritten. Lipfert reports used kept to limited number with specific information.	Kotchmar
		pp. 12-113 to 12-116 Table 12-1	062-12-4	Data given is a mixture of range and percentages.	Data is that reported in the studies.	Kotchmar
		p. 12-145 lines 20 and 21	062-12-5	What does this mean?	Sentence revised to make appropriate statement.	Kotchmar
		p. 12-169	062-12-6	This reads more as a critique than a summary.	Reorganized and deleted and revised aspects of discussion.	Kotchmar
		p. 12-177 lines 30 and 31	062-12-7	Avoid use of "investigator-dependent".	Deleted.	Kotchmar
		p. 12-305 Figure 12-34	062-12-8	The arrows suggest equal strength for each pathway. This is not the case.	Cautionary statement in this regard is added to title.	Kotchmar
		p. 12-318 Sec. 12.6.4.2	062-12-9	Discuss Godleski's work.	Section revised, but specific hypothesis, i.e., Godleski's work (not yet published) not discussed here.	Kotchmar
		Appendix 12B	062-12-10	Needs a concluding discussion.	Discussion blended throughout main text.	Kotchmar

# RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-029	Wolff	page 13-7, line 1	029-13-1	Are the nuclei and ultrafine modes identical.	It was incorrect usage to write "nuclei or ultrafine mode". Nuclei refers to a mode, an experimentally observed section of the atmospheric size distribution. However, ultrafine is used for any size below 100 nm or any size distribution with MMAD below 100 nm. In revised version, only fine and coarse modes discussed and ultrafine applied to particles, not modes.	Wilson
		page 13-7, line 22	029-13-2	Say something about the lifetimes of ultrafines.	Lifetime added . . . "<minutes". Section 1.3.2.4.	Wilson
		page 8, line 16	029-13-3	Elemental carbon is a fine particle that does not condense from the gas phase.	The sentence stating that fine particles condense from gases was eliminated when this section was condensed. However, we understand that much fine elemental carbon is formed by condensation of C <sub>2</sub> molecules formed in combustion.	Wilson
		page 13-8, line 27-28	029-13-4	"We have data for a number of sites on coarse EC."	Data not provided. Criteria Document uses only peer-reviewed, published (or accepted for publication) information.	Wilson
		page 13-8, line 28	029-13-5	Insert "of noncrustal origin" after "metals".	This sentence eliminated in condensation and rewrite of this section.	Wilson
		page 13-9, line 9	029-13-6	Change "hydrocarbons" to "volatile organic compounds".	This sentence eliminated in condensation and rewrite of this section.	Wilson
		p 13-34, lines 1 to 7	029-13-9	How can we be sure that mortality/morbidity effects will decrease if we reduce PM <sub>2.5</sub>	Comment noted. One would have to design and implement a study to examine this. Projected benefits of PM <sub>2.5</sub> reduction more appropriately addressed elsewhere (e.g., in staff paper).	Kotchmar
		p 13-44, lines 8 to 25	029-13-10	Except for Topeka, the relative risks are essentially the same for PM <sub>10</sub> and PM <sub>2.5</sub> .	The differences are best visualized in Figure 12-33. Also, Chapter 13 lists the PM <sub>10</sub> and PM <sub>2.5</sub> relative risks in Tables 13-3 and 13-4.	Kotchmar
		p 13-46, lines 1 to 5	029-13-11	See comment 029-12-9.	See response to comment 029-12-9.	Kotchmar
		p 13-52, lines 15 to 17	029-13-12	What is the basis for this statement.	Revised expanded coherence discussion. Deleted this paragraph.	Kotchmar
		p 13-64, lines 24 to 27	029-13-13	Doesn't this cast some doubt over the choice of PM <sub>2.5</sub> as the causal agent.	The paragraph goes on to state that the statistical power of the study was limited.	Kotchmar



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Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-029 (cont'd)	Wolf	p 13-73, lines 5 to 7, lines 9 to 12	029-13-14	Don't these statements undermine the selection of PM <sub>2.5</sub> as the causal agent.	Similar statements in the revised chapter are appropriate cautions. Undermine is not the word choice, caution is. The discussion in part refer to PM <sub>10</sub> and PM <sub>2.5</sub> .	Kotchmar
		p 13-79, lines 20 to 31, p 13-80, lines 1 to 3	029-13-15	Why not extrapolate a mean result or a meta-analysis to other places.	The first part of the paragraph discussed this. However, the revised chapter deleted this discussion.	Kotchmar
IIAF-034	Mauderly	Chapter 13	034-13-1	Must present adequate job of synthesizing data on the specific question of is there a need for a separate small particle standard in relation to PM <sub>10</sub> .	Chapter revised to present the related data and analysis. Additionally, new studies are discussed. The most appropriate discussion is presented based on the available studies.	Kotchmar
		p. 13-13, lines 7, 28, 1	034-13-2	2.5 µg should be 2.5 µm.	Corrected, deleted in revision.	Kotchmar
		p. 13-48, line 10	034-13-3	moles/m <sup>3</sup>	Deleted in revision.	Kotchmar
		p. 13-63, line 28	034-13-4	Thus sparing should be, thus reducing.	Clarification not evident in final document.	Kotchmar
		p. 13-71, lines 7 and 8	034-13-5	Eliminate this phrase. This is not a significant environment consideration.	Section deleted.	Kotchmar
		p. 13-77, line 9	034-13-6	Tracheal should be tracheal.	Deleted in revision.	Kotchmar
IIAF-035	Samet	Chapter 13	035-13-1	General statement on need for improvement of the integrated synthesis for the chapter.	The revised chapter represents more of coverage and synthesis aspects that were not as evident in the earlier draft.	Kotchmar
		p. 13-21, line 5	035-13-2	Inconsistency of efficiency for particles to penetrate indoors needs to be added.	Discussion revised and rewritten to more clearly evaluate this exposure-related area.	Kotchmar
IIAF-036	Urell	Chapter 13	036-13-1	Chapter 13 needs to be integrated further, and the discussion relating fine particles to health effects needs to be improved.	The integration of material was improved within the confines of the data. An improved discussion of the available data on the health effects of fine particles was presented.	Kotchmar
IIAF-037	McClellan	Chapter 13	037-13-1	The chapter needs to be revised. A section on health statistics needs to be added. A revised integrative synthesis is needed with a strong exposure effective dose response orientation. Derived metrics needed to be stated.	See response to comment 36-13-1. Health statistics appear in Chapter 12 and in Table 13-9.	Kotchmar

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# RESPONSE TO CASAC COMMENTS—PARTICULATE MATTER AQCD (NOVEMBER 1995 DRAFT)

Docket #	Member	Ch-Pg Line/Tab/Fig	Com #	Comment Summary	Response	Responder(s)
IIAF-043	Lippmann	Chapter 13	043-13-1	Over 20 specific suggested comments are provided.	CASAC requested the chapter be integrated more effectively than earlier draft. This was implemented. In doing so, major deletions and revisions occurred. Thus, some of the specific comments were changed in the final text by major rewrites. Where changes could be located they were implemented with the exception of numerical change on p. 13-47. These were, however, implemented as requested in Chapter 12.	Kotchmar
			043-13-2	Howath et al. is not an ultra fine study.	This is no longer referred to as an ultrafine study but the particle size is indicated as <100 nm, which is in the ultrafine range.	Folinsbee
IIAF-062	Speizer	Chapter 13	062-13-1	The chapter needs to be better integrated.	The chapter was revised and reorganized to be more integrative.	Kotchmar
IIAF-048	Stolwijk	General statement.			Noted.	Kotchmar

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